Letters to the Editor.

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Biological Terminology.

DR. BATHER insists (NATURE, June 16, p. 489) that systematic zoology and botany are not wholly based on description, and gives some interesting interpretations of his own. Of course he is right—as right as if I had said that Africa is a land mass, and he had retorted that there were lakes in it. Driven by necessity, we all, even systematic zoologists and botanists, and even from infancy, practise inference and seek to make sure. We employ crucial testing when we desire to ascertain whether an explanation is true. We neglect it (*e.g.* in favour of rhetoric) when we wish merely to convince ourselves or others that it is true—as in the case of politicians, theologians, and those 262 biologists who propounded 262 explanations of sex and did not attempt to test even one. But all serious scientific interpretation is governed by very stringent rules: we must found our suppositions on verifiable facts; we must try to think of all alternative explanations of those facts; and, lastly, we must seek fresh and unlike groups of facts which shall eliminate, one after another, all the erroneous explanations. Then, and not until then, shall we have finished with mere guessing. As Überweg puts it: "One single circumstance which admits of one explanation only is more decisive than a hundred others which agree in all points with one's own hypothesis, but are equally well explained on an opposite hypothesis."

Now, can Dr. Bather tell us of any modern sect of biologists which employs this method? It became fashionable among physicists and astronomers before Newton, and is still the very breath of their nostrils. Next it invaded chemistry—hence the rout of the alchemists. Then it captured physiology—hence the modern science. Darwin and some of his contemporaries tried to introduce it into biology. But with the passing of Darwin the impulse ceased. The new men proceeded, *unquestioned* (that is the damning point), to break every rule of scientific procedure. They coined multitudes of words that sounded tremendously scientific, but actually had no meanings in their mouths, *e.g.* germinal, blastogenic, plasmogenetic, somatic, and the like. They formulated hundreds of hypotheses, and argued about them strenuously, but—because of the vagueness of their principal terms (*e.g.* innate, acquired, inherit); because they rarely tested hypotheses and never as a body accepted a test; and because lack of crucial testing prevented the utilisation of oceans of unlike, but perfectly authentic and relevant, evidence that waited unexplored in a host of subsidiary sciences—their controversies were unending. Lastly, there happened the strangest event in the history of science. Groups of biologists, disgusted with the unceasing babble, declared that they were done with controversy, and founded the "exact" and "modern" schools. That is to say, each group, believing that a particular way of observing facts was especially modern and exact, proceeded to restrict its evidence to facts observed in that way. But, as we shall see presently, there is no especially accurate way of observing, and it is a fundamental axiom that all facts, no matter how observed, are equal before science. Again, if the area whence facts are derived be reduced, there is a corresponding reduction of evidence—of the power to discover crucial tests. Again, while controversy is unnecessary, there must be discussion, or the truth can never be established. There is a distinction between the two which implies a difference in temper. In controversy men try to belittle the facts and inferences of opponents; in discussion they candidly examine them with a view to ultimate agreement. Yet, again, "exact" and "modern" are rhetorical misnomers. This method of restricting evidence is very ancient. It has always fomented controversy, prevented discussion, and led, not to agreement, but only to the foundation and perpetuation of sects. Thus, Mohammedans have always used only Mohammedan evidence.

I am told that biologists think that I have wasted ink and paper on a subject—the method of science—which was thrashed out long ago. Certainly it has often been thrashed out; hence modern science. But never has it been thrashed out among biologists; hence the chaos in biology. As every biologist knows, his opponents, usually the majority, lack the right method; hence their incapacity to perceive the truth which to him is so plain. Anciently the scholastic thinkers founded their assumptions on unverified data and neglected to test them by fresh appeals to reality; hence the dark ages of Europe. Francis Bacon and his successors insisted that hypotheses must be both founded on, and tested by, verifiable data; hence modern thought and civilisation. But biology is still in the pre-Baconian stage. It is founded mainly on the unverified assumptions that some characters are more acquired, or innate, or inheritable than others, and, as I say, biologists rarely test their suppositions, and never as a body accept tests; hence the persistence, in great measure, of the dark ages in modern society. To-day no obscurantist dares to meddle with the established truths of astronomy, geology, or any interpretative science save biology. But he is still supreme in all that pertains to life. For example, he controls education, and, having rendered men unintelligent and trained them to unreason and passion, has recently drenched the world in blood. In England a million people, many of them innocent in every sense, are poisoned annually by means of easily preventable venereal disease, because ferocious, but ostensibly saintly, savages desire to punish sin. Yet man is a living being, and after all these years biologists should be able to tell us, with the full force of established truth, what may be achieved by education and how to achieve it. At present, notwithstanding the work of Lankester and others, biologists are impotent. However, it will not always be so. Sooner or later they are sure to fall into line with other scientific workers, and found one of the greatest and most potent of sciences.

I daresay biologists will think I am vapouring, for most of them are zoologists and botanists, and do not, almost as a point of honour, look outside their special sciences; and, while all biologists will agree that their opponents (usually, as I say, the majority) employ wrong methods of inquiry, none will believe that biologists as a class are ignorant or neglectful of the right method. Well, consider the following. Scores of similar instances may be found in literature. Once I read a book in which the author formulated suppositions of no very great importance, but which he, apparently using all the available evidence, tested carefully and established successfully. I may have been wrong in my opinion, and the author may have been superficial; but later I read a review of the book by a very distinguished biologist. He controverted not a fact or an inference; all he had to say was: "The trail of the deductive thinker is over all." But how on earth can any supposition be established except by deduction? Crucial testing *is* deduction. All interpretative science has been created by it. "If my hypothesis be true and all others wrong, then this thing, and that, and that other must be true also. Let me see if it is so." Again, I once argued with another distinguished biologist. "But isn't the evidence true?" said I. "It may be," said he; "but it's not the sort of evidence we accept nowadays!" I found that his opinions corresponded with those of the writer of the following passage, another very distinguished biologist:—

"The recognition that only by experimental methods can we hope to place the study of zoology on a footing with the sciences of chemistry and physics is a comparatively new conception, and one that is by no means admitted as yet by all zoologists. I do not wish to disparage those studies that deal with the descriptive and the historical problems of biology. . . . It is undoubtedly true that many zoologists who have spent their lives in acquiring a broad knowledge of the facts of their science fail to make use of their information by testing the very problems that their work suggests. This is owing, no doubt, to their exclusive interest in the observational and descriptive sides of biology, but also in part, I think, to the fact that the experimental method has not been recognised by zoologists as the most important tool that scientists employ . . . the essence of the experimental method consists in requiring that every suggestion (or hypothesis) be put to the test of experiment before it is admitted to a scientific status. From this point of view the value of a hypothesis is to be judged, not by its plausibility, but by whether it meets the test of experiment. . . . It is sometimes said that Nature has already carried out innumerable and wonderful experiments. . . . Let us not be blinded by rhetorical questions of this kind." And so on, and so forth.

I wonder if anyone can tell us of a passage in any sort of literature which contains more misunderstanding than the one I have quoted. "Rhetorical" indeed! *Of course,* the essence of the experimental method is *not* that every suggestion shall be put to the test of experiment. Experiment is a mode of observing, not of thinking. It is used only when the desired facts cannot be directly observed, and only for the purpose of removing the conditions which obscure them. *Of course,* physics and chemistry are advanced and accurate, not because of difficulties in observing, but only because their workers have verified both their facts and their thinking, because they have accepted all tested suppositions readily, and because they have been in a position to measure and weigh accurately. They are experimental only because, from the nature of things, most of the desired facts were obscured and could not be revealed except by some device. Physicists and chemists would not be so silly as to experiment if they could observe at sight. As a fact, nearly all their established truths started with suppositions founded on patent facts, and were tested experimentally only because of the paucity of such evidence. There is nothing sacrosanct about a fact discovered experimentally, or about a hypothesis so tested. If you twist a dog's tail experimentally, the howl you elicit is not in the least more valuable intrinsically than the tail which you observe directly. You prove a man a thief just as surely by observing the spoons sticking out of his pockets as by turning them out. *Of course,* experiment is valuable; but we need not assign it wrong values. It enables us to penetrate below the surface. But a diving dress is not the only wear. There are things worth knowing on the surface—so many things, nearly all the facts of zoology, botany, and most other biological sciences, that if they be ignored, crucial testing is impossible. *Of course,* hypotheses, founded on or even confirmed by experiment, are, like all other untested hypotheses, mere guesses; whereas all tested hypotheses, whether proved experimentally or otherwise, are in a different category: for a real test is crucial; it not only confirms the truth, but also eliminates the untruth. If literature be examined I think it will be found that when anyone insists that all suppositions must be put to the test of experiment, he really asks that we shall ignore all evidence except that revealed by experiment, and all suppositions except those founded on experiment—that, in fact, we shall grant his facts the same status and his opinions the same immunity from criticism that other sectarians (*e.g.* Mohammedans) claim for their evidence and opinions. Compare Newton, who when he found that the moon's orbit (a thing which must be directly observed) did not, as then calculated, fit into his theory of gravitation, laid aside his supposition for many years, and published it only when a fresh inquiry demonstrated an error in the first calculation. Consider the glaring truism that "Variation is the sole cause of non-inheritance; apart from variations, like exactly begets like when parent and offspring develop under like conditions." No biologist will venture to dispute that truism. Is it necessary to test it experimentally? If it be true, what becomes of the Lamarckian and Neo-Darwinian suppositions, and much besides that biologists have unendingly "tested" experimentally and unendingly disputed about?

When such freakish, partisan stuff as I have quoted can be published and applauded, is it not evident that biology must remain a tumbling ground for whimsies unless its workers discuss and agree on its rules of procedure? The rules under which other interpretative sciences have been created—that language must be precise and significant, that all verifiable facts are equal before science, that all suppositions must be crucially tested before admittance to a scientific status, and that all fully tested suppositions must be candidly accepted—are so few and simple that were they rigorously applied, then, since the evidence is so abundant, it is certain that biology would soon rank among the greatest, best established, and most useful of sciences. It is generally thought that the multiplicity and diversity of biological phenomena, derived, as they are, or should be, from so many sciences, make the task of the inquirer peculiarly difficult. Actually the reverse is the case; for, when evidence is abundant and diverse, opportunities for testing are correspondingly frequent and established truth, rightly sought, should soon cover a wide area. Other sciences halt while evidence is painfully collected; biology halts because the methods of its students are such that they are unable to use the abundant evidence already available.

Dr. Bather says *à propos* of recapitulation: "At the moment when his letter was published some of us were discussing that very question at the Linnean Society, and Sir Archdall Reid, had he been present, would have seen that the issue was far from being the simple one that he imagines." As readers of NATURE know, I am very modest and retiring. Nevertheless, if Dr. Bather will indicate his difficulties I believe I can help him. The truth is, I have, on one hand, great faith in ordinary scientific procedure, and on the other, amid vast ignorance, a knowledge of some facts which appear to have escaped the notice of biologists; for example, the truism I have mentioned, that, apart from variations, offspring tend to recapitulate the development of their parents.

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Solar Eclipse Results and the Principle of Relativity.

On a recent occasion I read a paper before the Manchester Literary and Philosophical Society on the nature of dimensions, in which, admitting the possibility and even probability of space and time having secondary characteristics, like those suggested by Lorentz and Einstein, reasons were given for doubting whether the methods employed for finding them could be relied on, and experimental evidence, before it could be accepted, would have to be subjected to searching adverse criticism. Prof. Eddington's solar eclipse results were therefore submitted to a process essentially the reverse of his, which had for object not the confirmation of a theory, but the discovery of an empirical relation. During this process it soon became evident that the astigmatism of the cœlostat mirrors, which had given much trouble during the eclipse by distorting the star images, had also affected the field and altered the star positions. The stopping down of the objectives aggravated this evil in a double sense: first, the reduction of the star-image astigmatism makes it impossible to construct a picture of what might be called the field astigmatism of the mirrors; and, secondly, the smaller the diameter of the pencil of light rays for each star, the further apart would be the regions on the mirror from which these pencils were reflected. Therefore, if the various regions of the mirror had semi-independent tilts, the places of the stars on the plates would be affected by these tilts. If for some of the existing plates these pencils should be found to overlap, and if the star-image astigmatism is sufficiently marked, then these plates might still be used for the object for which they were taken.

Wishing, first of all, to redetermine the positions of the stars as they appeared before scale corrections had been applied in order to trace the wandering of the images, it was found that for the outermost stars 10 and 11 these negative corrections would amount to 0·45″. This in itself was a most disconcerting discovery, for the difference of displacement on which the eclipse conclusions were based is only 0·75″. It is, of course, quite impossible for the telescope tube to have altered sufficiently in four and a half minutes to produce this result, and a change of focus of the objective would have had no effect; therefore the mirror must have warped even during this short time. At Principe the passage of clouds would produce an irregular warping effect, and this would account for the unsatisfactory photographs obtained there. In order to form some idea about the nature of the field astigmatism it was now decided to estimate the tangential displacements of the stars, for these would not be influenced either by the scale corrections or by the radial displacements, but only by the mirror warpings. It was then found that stars 3, 4, and 6, which lie almost in a straight line between 5 and 10, had moved about 0·45″ across the connecting line of these outer stars. On averaging the uncorrected radial displacements it was found that a slight scale correction of about +0·0002″ had to be made, as was done by Prof. Eddington, in order to harmonise them with the Einstein estimates, the excuse in my case being that the mirror may have acquired a slight temporary concavity. If, however, there was a temporary convexity, necessitating a scale correction of, say, −0·0003″, the displacements would appear to conform to the empirical formula 1·09″−0·00022″. It will thus be seen that the use of cœlostat mirrors is not advisable where, as under eclipse conditions, rapid changes of temperature are unavoidable.

C. E. Stromeyer.

"Lancefield," West Didsbury, July 21.

Mr. C. E. Stromeyer greatly exaggerates the possible effects of astigmatism of the cœlostat mirrors on the positions of the star images. He appears to consider that the pencils forming the different images are reflected from entirely different portions of the cœlostat surface. As a matter of fact, with the 4-in. lens the pencil producing the extreme star image was reflected from a portion of the mirror which had 85 per cent. of its area common to that producing the central pencil, and with the 13-in. lens (stopped down to 8 in.) the common portion was even greater.

That there was a slight astigmatism of the mirrors is not denied. Its presence was indicated by small differences in the scale and orientation constants of the plates determined in two different ways: from the right ascensions or declinations. These differences gave a measure of the amount of the astigmatism, and showed it to be very small and of very slight effect on the gravitational displacement. The question has been fully discussed by Prof. H. N. Russell (Monthly Notices, R.A.S., vol. lxxxi., No. 2, December, 1920), with the result that the conclusions deduced from the original reductions of the eclipse plates were fully substantiated and the Einstein displacement confirmed.

F. W. Dyson.

The Atomic Radius and the Ionisation Potential.

Prof. Eve's interesting contribution to Nature of June 30, p. 552, on the relation between the ionisation potential and the atomic radius induces me to publish certain similar ideas of mine on the same subject to which I referred some time ago before the Royal Society in some remarks on Prof. Rankine's paper "On the Proximity of Atoms in Gaseous Molecules" (Proc. Roy. Soc., February, 1921). I did not publish the results, because I desired to wait for further data. These ideas may be stated as follows:—

According to the Rutherford-Bohr model of the atom, by the radius of the atom is meant the distance from the nucleus of the outermost electron, *i.e.* the electron the quantum vibrations of which cause the radiation of the arc lines of the atom. Sommerfeld has shown that in the normal (unexcited) state the orbit is characterised by the azimuthal quantum-number unity and the radial quantum-number zero. This orbit is circular, but to calculate its radius we must know what is the field of force exerted by the central nucleus and the remaining $(n-1)$ electrons upon the vibrating electron. This is at present an insoluble problem, but Mr. S. N. Basu (*Phil. Mag.*, November, 1920) has shown that we can at least arrive at a qualitative explanation of Rydberg's laws of spectral regularity by assuming the attracting system to be equivalent to a net central charge unity, with a doublet of strength L. On the bases of this theory, if a_x is the radius of the monoquantic orbit, it is easy to show that

$$\frac{e^2}{2a_x} = -\text{energy of the vibrating electron.}$$

$= h(1s)$ where $(1s)=$ convergence frequency of the principal series of the element in absolute measure.

$= eV_x$, $V_x =$ ionisation potential in *e.s.* units.

For the H-atom we have, according to Bohr's theory;